Different Views

The Effectiveness of Automobile Safety Regulation: Evidence from the FARS Data

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Abstract: In a paper published in the August 1981 issue of this Journal, Leon Robertson attempts to measure the effects of the vehicle safety and occupant protection standards implemented in the 1960s. Data from the Fatal Accident Reporting System are used. Additional statistical analysis with these data reveals a multicollinearity problem that makes the prediction of the effects of regulation uncertain. There is also bias in regression results due to the inappropriate inclusion of truck data in the regressions. Regressions on the car data reveal a lifesaving effect of regulation that, at best, is one-fourth the value reported by Robertson. *(Am J Public Health* 1984; 74:1384–1389.)

Introduction

In the August 1981 issue of this Journal, Leon Robertson published the results of research on the effectiveness of various automobile safety standards using data from the Fatal Accident Reporting System (FARS).[1] On the basis of his statistical work, Robertson calculates that the various occupant protection and crash avoidance standards initially implemented between 1964 and 1968 were responsible for preserving approximately 37,000 lives during the calendar years 1975 through 1978.

Robertson has kindly made data used for his research available to me. On the basis of my analysis of these data, I propose to offer some alternatives to Robertson's procedures, and to the conclusions that are based on them.

A careful reading of Robertson's paper raises three related issues.

1. What is the underlying structure represented by the regression equations?
2. Why are truck data included in regressions when Robertson's primary purpose is to measure the effect of regulations imposed almost exclusively on cars?
3. Five of the six independent variables, including the two regulatory dummy variables, are vehicle age variables. How stable are the coefficients used to estimate the effects of regulation, given the potential collinearity problem that this creates?

I will try to provide a clear and compact analysis of these issues by developing regression equations along a path similar to the one taken by Robertson.

Methods

In attempting to assess the lifesaving effect of regulation on car occupants and various groups of non-occupants with which they collide, I want to compare regulated cars with unregulated cars—controlling for other determinants of automobile accident death rates. Environmental factors, one assumes, will be approximately the same for the various model years of cars involved in fatal crashes. The physical condition of the cars will be of concern, as will possible differences among drivers in their risk-taking propensities. Risk-taking propensities might be captured by various socioeconomic variables, a few of which are available in the FARS data.* Robertson does not use these variables in his regression equations. Whether or not these factors could be productively used is a question for further research, and not an important issue in this critique.

The implicit assumption in Robertson's paper is that important determinants of death-causing accidents are functions of vehicle age, so that age may be used as a proxy for these variables. And, in the case of cars, it is an impressive proxy (Table 1). The age polynomial used by Robertson explains 97 per cent of the variance in occupant deaths rates and 98 per cent of the sum of occupant and three other categories of death rates.** A linear age variable does nearly as well by itself, explaining 96 per cent of the variance in the dependent variable.

One of the age correlates is safety regulation. Newer cars are more heavily regulated than older ones; particularly important regulatory changes took effect in 1964 and 1968. Following Robertson, we attempt to extract the variance in death rates attributable to these regulations by adding two dummy variables. As explained by Robertson, the state-GSA (General Services Administration) regulatory variable is zero prior to 1964 and one thereafter for cars, and the Federal regulatory variable is zero prior to 1968 and one

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*It is difficult to interpret Robertson's cross-tabulation of these variables. Driver age is very crudely measured, and the covariation between variables such as age, previous crashes, and violations is ignored. The relevant covariation between these variables, the dummy variables representing regulation and the vehicle age variable, is never determined. Important socioeconomic variables such as income and education are not available in the FARS data.

**Robertson's death rates are deaths per 100 million vehicle miles as calculated separately for cars and trucks in each calendar year. The data provided by Robertson did not include his "all involvements" death rate. This is of little concern since the regression based on it has only marginal relevance and is not analyzed in Robertson's own paper.

TABLE 1—Age of Vehicle as a Determinant of Death Rates for Cars and Trucks (t values in parentheses)

Death Rate	A	A^2	A^3	Adjusted R^2	F
Cars (n = 60)	0.161	—	—	.96	1452
	(38.10)				
Occupants	0.208	−0.003	—	.97	821
	(12.23)	(−2.88)			
	0.034	0.023	−0.001	.97	748
	(0.84)	(4.03)	(−4.60)		
	0.213	—	—	.96	1264
	(35.56)				
Occupants plus	0.318	−0.007	—	.97	908
Non-Occupants	(14.75)	(−5.02)			
	0.060	0.032	−0.002	.98	964
	(1.26)	(4.77)	(−5.81)		
Trucks (n = 60)	0.032	—	—	.43	45.0
	(6.71)				
Occupants	0.069	−0.002	—	.45	25.4
	(3.46)	(−1.91)			
	−0.156	0.32	−0.001	.63	34.6
	(−3.47)	(4.93)	(−5.35)		
	0.032	—	—	.27	22.8
	(4.78)				
Occupants plus	0.108	−0.005	—	.35	16.8
Non-Occupants	(3.93)	(−2.83)			
	−0.229	0.046	−0.002	.60	30.5
	(−3.85)	(5.44)	(−6.07)		

thereafter.*** However, before discussing the results of adding these variables to our regressions, we need to consider the issues of trucks and collinearity.

There are 60 observation points for cars, consisting of four calendar years of fatal crashes (1975–78) and 15 car model years within each calendar year. Robertson extends the observations to 120 by including truck death rates and adding a car-truck dummy to the regressions. Robertson does not explain his reasons for including truck data in his regressions but it is possible to evaluate the effect of their inclusion relative to the primary purpose of the research—the investigation of safety regulations that were applied to cars.

Although observations on cars constitute an obvious and defensible data set, one can argue that trucks, remaining free of the safety regulations applied to cars, could serve as a control group along with unregulated cars. However, the validity of such an exercise depends on the ability to capture within the regressions the non-regulatory determinants of the differences between car and truck death rates. There is no theoretical structure to inform us of these differences. There is only the car-truck dummy, which shifts the intercept coefficient in regressions where it is used, and the coefficients of the age polynomial that are jointly estimated for cars and trucks.

There are good reasons to expect substantial differences between cars and trucks in the structure of the age polynomial. Some determinants of the expected differences are:

- the wide diversity in vehicles labeled as trucks;
- the substantial physical differences between trucks and cars and the differences in their drivers;
- the substantial differences in truck use compared to cars;
- the exemption of trucks from occupant protection safety regulation; and
- the much lower overall death rates associated with trucks.

In a regression on cars and trucks it is possible to capture at least some of the non-regulatory differences with the car-truck dummy. The effects of regulation are estimated with the regulatory dummies. However, in the Appendix it is demonstrated that unexplained structural differences between the car and truck populations remain. After allowing for the effects of regulation in the car regression, and entering the car-truck dummy in the pooled regression, the hypothesis that the coefficients of the age polynomial are the same for cars and trucks is rejected with a very high level of statistical significance.

An additional problem raised by the inclusion of truck data is that the car-truck dummy introduces what is perhaps an unexpected source of collinearity. The two regulatory dummy variables and the car-truck dummy variable always equal zero for trucks. The car-truck dummy always equals one for cars. The federal regulatory dummy and the state-GSA regulatory dummy are equal to one 57 per cent and 83 per cent of the time, respectively, for cars. Otherwise, they are zero. Thus there are built in positive correlations of .63 and .85 respectively between the car-truck dummy and the regulatory dummies. These are the highest simple correlations among the independent variables, except for the correlations among the three powers of vehicle age.

Thus the inclusion of truck data is not justified by any theoretical or empirical underpinnings. Indeed, there is good reason to believe that their inclusion creates serious problems in regressions that attempt to measure the effects of automobile safety regulation. The multicollinearity problem raises questions about the interpretation to be given specific regression coefficients that are crucial to the measurement of lives saved. The combination of two importantly dissimilar populations, with different parameter values for common variables, creates misspecification and raises the question of bias in parameter estimates. The problem is that the coefficients of the regulatory dummies may be substantially biased, and they may be measuring a substantial amount of irrelevant variance between car and truck death rates, rather than the relevant variance between regulated and unregulated vehicles. For these reasons, a procedure that limits the comparison to regulated and unregulated cars is necessitated by the limitations of the FARS data that Robertson employs.

Results

The first four segments of Table 2 compare Robertson's regressions using pooled car and truck data with regressions using car data only.† The changes from Robertson's results are substantial. The overall statistical fits are improved due to the separation of two dissimilar populations. The coefficients of the regulatory variables are markedly reduced with

***As a result of laws passed in 14 states, manufacturers installed front outboard lap belts in all cars beginning in 1964. The General Services Administration (GSA) issued standards for vehicles purchased by the United States Government beginning in 1966. These changes were also applied to some models sold to the general public. The 1968 regulations were the initial standards required by the National Traffic and Motor Vehicle Safety Act of 1966.

†The cars and trucks regression coefficients (n = 120) show some very minor variations from those published by Robertson.

TABLE 2—Robertson's Regression's and Regressions on Cars Only

Death Rates	Federal Regulations	State & GSA Regulations	Car/ Truck	Vehicle Age A	Vehicle Age A^2	Vehicle Age A^3	Adjusted R^2	F
Occupants (CT)	−0.72	−0.36	1.38	0.030	0.012	−0.0008	.90	188
t	(−10.25)	(−3.99)	(17.68)	(0.59)	(1.63)	(−2.46)		
Occupants (C)	−0.18	−0.04	—	0.084	0.015	−0.0008	.98	501
t	(−2.85)	(−0.64)		(1.93)	(2.22)	(−2.85)		
Pedestrians (CT)	−0.21	−0.07	0.27	0.005	0.004	−0.0003	.71	49.4
t	(−7.75)	(−2.16)	(8.99)	(0.26)	(1.44)	(−2.45)		
Pedestrians (C)	−0.07	−0.05	—	0.016	0.007	−0.0004	.93	152
t	(−2.65)	(−1.72)		(0.88)	(2.44)	(−3.79)		
Motorcyclists (CT)	−0.06	−0.01	0.03	0.010	0.001	−0.0001	.26	8.0
t	(−2.57)	(−0.24)	(1.06)	(0.63)	(0.28)	(−0.74)		
Motorcyclists (C)	0.046	0.12	—	0.017	−0.0004	0.00006	.50	12.6
t	(1.33)	(3.15)		(0.70)	(−0.12)	(0.41)		
Pedalcyclists (CT)	−0.03	−0.004	0.03	0.001	0.0001	−0.00001	.28	8.8
t	(−3.31)	(−0.36)	(3.53)	(0.25)	(0.16)	(−0.32)		
Pedalcyclists (C)	−0.02	0.005	—	0.013	−0.001	0.00004	.54	14.9
t	(−1.80)	(0.43)		(1.91)	(−1.21)	(1.01)		
Occupants plus Non-Occupants (CT)	−1.01	−0.44	1.71	0.046	0.017	−0.001	.88	153
t	(−10.34)	(−3.55)	(15.7)	(0.65)	(1.65)	(−2.64)		
Occupants plus Non-Occupants (C)	−0.22	0.03	—	0.129	0.020	−0.001	.98	658
t	(−3.02)	(0.38)		(2.58)	(2.58)	(−3.50)		

CT: (Robertson) Cars and Trucks n = 120.
C: Cars only, n = 60.

the removal of the truck data and the car-truck dummy. The state-GSA regulatory variable is never statistically significant, except in the case of motorcycles where it has the wrong sign. The federal regulatory variable maintains its negative sign and is statistically significant except in the case of motorcycles. The "occupant plus non-occupant" death rate regression was not run by Robertson, but by summing the death rates in the first four categories and regressing the results on the independent variables we get a summary of the effects for the included categories of fatalities. Assuming that Robertson's calculation of fatalities is proportional to the size of his estimated coefficients, I compute a figure of 5,000 lives saved over the four-year period 1975–78.†† This contrasts sharply with Robertson's estimate of 37,000 lives saved.

Eliminating the truck data and car-truck dummy does not eliminate the collinearity problem. The potentially troublesome collinearity is now demonstrated in correlations ranging from −.62 to −.85 between the regulatory dummy variables and the age variables. A growing practice among empirical researchers is to publish a range of coefficient estimates from their empirical work. This practice gives some measure of the robustness of results to the existence of collinearity, errors in measurement, misspecified structure,

††$\frac{(-0.22 + 0.03)}{(-1.01 - 0.44)}$ 37,100 = .131 × 37,100 = 4861

TABLE 3—Behavior of Coefficients for Regulatory Variables with Alternative Polynomials (t values in parentheses)

Age Variables Used	Occupant Death Rate: Federal Regulation	Occupant Death Rate: State & GSA Regulation	Occupant Death Rate: Adjusted R^2	Occupant plus Non-Occupant Death Rate: Federal Regulation	Occupant plus Non-Occupant Death Rate: State & GSA Regulation	Occupant plus Non-Occupant Death Rate: Adjusted R^2
A	−0.19	0.16	.97	−0.22	0.36	.97
	(−3.24)	(2.88)		(−2.88)	(5.06)	
	−0.20	—	.97	−0.23	—	.96
	(−3.12)			(−2.51)		
	−0.26	0.02	.97	−0.33	0.12	.98
A,A^2	(−4.36)	(0.27)		(−4.70)	(1.44)	
	−0.26	—	.97	−0.36	—	.98
	(−4.61)			(−5.18)		
	−0.18	−0.04	.98	−0.22	0.03	.98
A,A^2,A^3	(−2.85)	(−0.64)		(−3.02)	(0.38)	
	−0.18	—	.98	−0.22	—	.98
	(−2.83)			(−3.08)		
	−0.18	−0.03	.98	−0.23	0.04	.98
A,A^2,A^3	(−2.89)	(−0.47)		(−3.01)	(0.50)	
A^4,A^5	−0.18	—	.98	−0.23	—	.98
	(−2.90)			(−3.01)		

missing variables, etc.—problems that are present to some degree in all empirical work. It also provides some protection against the accusation that the results published are limited to those most favorable for a particular point of view.[2,3]

Table 3 reports on a modest effort in this direction, using data on cars. The coefficients of the regulatory variables are tested with age polynomials of varying degrees. The result of this limited exercise is fairly clear. The coefficients of the state-GSA regulatory variable are erratic, and occasionally statistically significant with the wrong sign. However, the coefficients of the federal regulatory variable are always statistically significant with the expected sign, and remain within a remarkably narrow range.

Using the coefficients in Table 3, a range of estimates for lives saved can be calculated from the regression coefficients. This is done by taking these coefficients as proportions of Robertson's regression coefficients (Table 2, CT) and multiplying by Robertson's estimate of lives saved. This can be done either with the attitude of "letting the chips fall where they may" (having performed the sensitivity analysis reported in Table 3) or by arguing for the elimination of the coefficients for the state-GSA dummy variable. The latter could be argued on the grounds that the coefficients usually are not statistically significant, and where they are positive and significant they are (clearly!?) substituting for a variety of non-linear age effects on death rates. It should also be noted that there are only 10 data points for cars prior to the state-GSA regulations and that the unregulated cars were all more than 11 years old at the time of the observations. However, these arguments are *ex post* a sensitivity analysis that reveals results that are other than traditional views on safety mandates would lead us to expect. Their validity cannot be determined within the context of the present data, and one could just as easily marshal arguments in the other direction.

The calculation of lives saved in Table 4 is performed using coefficients from regressions both with and without the state-GSA dummy variable. For reasons given in the previous paragraph, it is my view that excluding the state-GSA dummy *ex post* the sensitivity analysis gives arbitrary weight to the conclusion that regulation has been effective in saving lives. The most optimistic calculation of 9,200 lives saved over the four-year period is less than 25 per cent of Robertson's estimate. The reader can readily make other comparisons from the table.

The negative sign in Table 4 indicates the possibility of no effect, or a fatality increase following regulation. Table 5 gives the breakdown of the regression from which the estimate of fatality increase was derived. These regressions are consistent with a very strong version of risk compensation theory—a limited decrease in occupant death rates that is more than offset by an increase in non-occupant death rates.††† This is in sharp contrast with Robertson's interpretation of his own regressions in the next to last paragraph of his paper. The regressions in Table 5 are not particularly persuasive. Like the 9,200 lives saved (Table 4), they represent one extreme of the sensitivity analysis. However, all regressions on cars show a limited effectiveness of safety mandates, which is the most general prediction from Peltzman's theory.[7] They also show an increase in motorcyclist death rates (see Table 2, car regressions), the group of non-occupants most sensitive to a decline in driver vigilance.

Figure 1—a graph relating automobile age to occupant death rates—provides a visual summary of what the car data reveal. The close relationship between vehicle age and death rates is clear. The wide bars are from Robertson's Figure 1 where he initially invites us to conclude that regulation has been very effective in saving lives. They are plotted at the median point of the data used in their construction. The bars are seen as nothing more or less than a simple measure of the relationship between car age and death rates. The solid bars are the coefficients from Robertson's occupant regressions for the federal (−.72) and state-GSA (−.36) regulatory dummies. They represent his point estimates of the effects of regulation. They are plotted at the median car age at which the regulations were imposed. It is clear that these large shifts in the age-death rate relationship for cars never

†††This result is not central to risk compensation theory (in contrast to Robertson's implication when he refers to Peltzman's work in the third paragraph, and in the next to last paragraph of his paper). It is simply one of many empirical implications of a much broader theory. The reader interested in risk compensation theory and its possible relevance for traffic safety policy should see references 4–10, especially 10 for a broad perspective. Some important empirical work on this controversial issue is contained in references 7 and 10–16.

TABLE 4—Number of Lives Saved by Automobile Safety Regulation: (1975 through 1978) A Range of Estimates from the Car Data

	Including Coefficients of GSA-State Regulation		Excluding Coefficients of GSA-State Regulation	
	Occupant Death Rate	Occupants plus Non-Occupant Death Rate	Occupant Death Rate	Occupant plus Non-Occupant Death Rate
Minimum Estimate	700	−3600	4400	5600
Maximum Estimate	5900	5400	6400	9200
Robertson's Estimate (Cars & Trucks)	26,500	37,100	—	—

TABLE 5—Regressions on Cars Using the Linear Age Variable

Death Rates	Federal Regulation	State & GSA Regulation	Vehicle Age	Ajusted R^2	F
Occupants	−0.19	0.16	0.15	.97	635
t	(−3.24)	(2.88)	(20.07)		
Pedestrians	−0.05	0.12	0.04	.84	106
t	(−1.67)	(3.86)	(8.78)		
Motorcyclists	0.04	0.08	0.02	.49	19.8
t	(1.21)	(2.83)	(5.54)		
Pedalcyclists	−0.01	0.01	0.004	.54	23.7
t	(−1.14)	(1.23)	(3.68)		
Occupants plus Non-Occupants	−0.22	0.36	0.21	.97	670
t	(−2.88)	(5.06)	(21.71)		

occurred. In fact, it is difficult to see any break in the data pattern. Table 3 illustrates that replacing the linear age variable with an age polynomial results in some increase in the net estimated effectiveness of safety regulation. Since we do not know whether this is the result of improved specification or collinearity, the range of estimates in Table 4 is the appropriate representation of the analysis.

Discussion

The results from regressions on cars lead to one of two conclusions. The first is that the data are at least marginally adequate for the examination of safety regulation effects, and that those effects are very small. The range of estimates reported here are much closer to Peltzman's conclusion[7] of no effect (derived from an entirely different structure, different data sources, and a different time period) than to Robertson's estimates. An alternative conclusion is that the data and the techniques that were used are not adequate to isolate the effects of regulation. FARS data for additional calendar years are now available. It may be possible with more degrees of freedom, different techniques, and different measures of regulation to improve on the quality of the present results. But that is a matter for additional research.

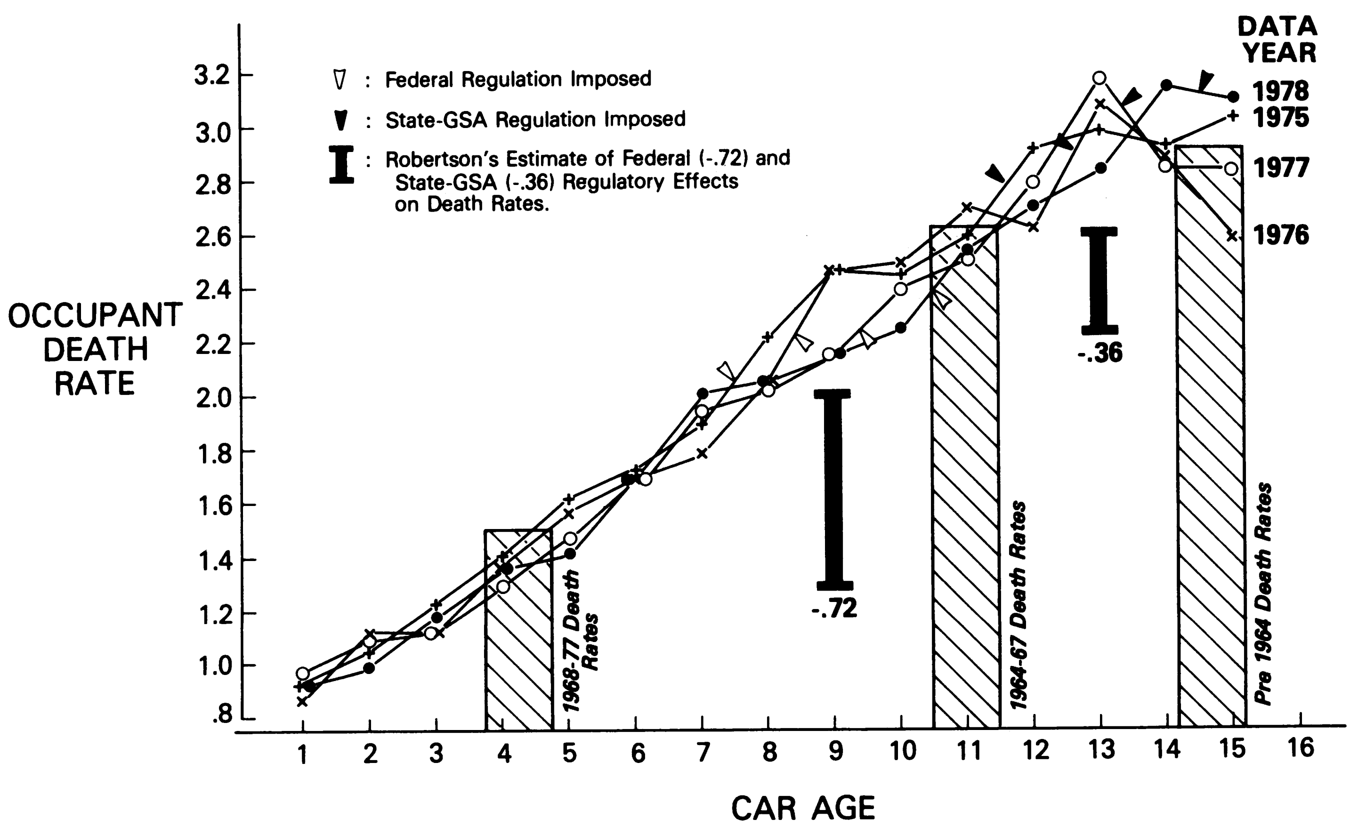

FIGURE 1—Car Age and Occupant Death Rates

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APPENDIX

In order for truck data to be useful in determining the effects of safety regulations applied to cars, the non-regulatory determinants of truck death rates must be the same as for cars, or the differences must be adequately estimated with variables included in the regressions. We can evaluate the appropriateness of Robertson's[1] inclusion of truck data in his regressions by running separate regressions on cars and trucks, and then combining the data for a pooled regression. The residual sum of squares for the two unrestricted regressions and the pooled regression provide the basis for a joint test of the hypothesis that coefficients of the age polynomial are the same for cars and trucks. Differences in the population death rates that are estimated by Robertson's three dummy variables are included in the regressions.

The structure of the dummy variables is:

		Regulation	
	Car-Truck Dummy CT	State-GSA Dummy S	Federal Dummy F
Trucks	0	0	0
Pre 1964 Cars	1	0	0
1964–67 Cars	1	1	0
1968–77 Cars	1	1	1

The unrestricted equation for trucks regresses the truck death rate on the age polynomial.

$$DR_T = \alpha_T + \beta_{1T}A + \beta_{2T}A^2 + \beta_{3T}A^3 + \epsilon_T$$

The unrestricted equation for cars regresses the car death rate on the regulatory dummies and the age polynomial.

$$DR_c = \alpha_c + (\alpha'' - \alpha_c)S + (\alpha''' - \alpha'')F + \beta_{1c}A + \beta_{2c}A^2 + \beta_{3c}A^2 + \epsilon_c$$

The pooled regression combines the car and truck data and includes the car-truck dummy variable. The coefficients provide k linear restrictions, $\beta_{1T} = \beta_{1c}$, $\beta_{2T} = \beta_{2c}$, and $\beta_{3T} = \beta_{3c}$.

$$DR = \alpha + (\alpha' - \alpha)CT + (\alpha'' - \alpha')S + (\alpha''' - \alpha'')F + \beta_1A + \beta_2A^2 + \beta_3A^3 + \epsilon$$

We have $n_T = 60$, $n_c = 60$, $k = 3$ age variables and $l = 2$ regulatory dummies. The sum of squared residuals, degrees of freedom and residual variances from the above regressions are:

The appropriate F test for the restrictions on the β coefficients is:

$$F = \frac{[SSR_P - (SSR_T + SSR_c)]/k}{(SSR_T + SSR_c)/(n_c + n_T - 2k - l - 2)}$$

Occupant Regressions:

$$F = \frac{[4.2117 - (.9031 + .6341)]/3}{(.9031 + .6341)/110} = 63.79$$

Occupant plus Non-Occupant Regressions:

$$F = \frac{[8.1698 - (1.5781 + .8560)]/3}{(1.5781 + .8560)/100} = 86.40$$

The hypothesis that the coefficients for the age polynomial are the same for cars and trucks is rejected with a very high level of statistical significance. The test assumes that the standard error of estimate is the same for both the car and truck regressions. For the occupant regressions this assumption cannot be rejected at the .10 level of significance. For the occupant plus non-occupant regressions it cannot be rejected at the .02 level of significance. With such large F values a moderate violation of the equal variance assumption would not upset the conclusion.

In summary the inclusion of truck data introduces bias, because the non-regulatory determinants of truck death rates are different from those for cars. These differences are not adequately estimated by the car-truck dummy.

	Occupants			Occupants plus non-occupants		
	SSR	d.f.	SEE^2	SSR	d.f.	SEE^2
Truck	.9031	$n_T - k - 1 = 56$	.0161	1.5781	56	.0282
Car	.6341	$n_c - k - l - 1 = 54$	.0117	.8560	54	.0159
Pooled	4.2117	$n_T + n_c - k - l - 2 = 113$	.0373	8.1698	113	.0723